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ABSTRACT

Prevailing criticism of communication research in and out of university speech departments is justified and means that the quality of research must be improved. The last three years of speech communication research has not been noteworthy, dramatic, or significant. An initial remedial step might be the development of good, relevant, and sound research questions. The science of communication must be conceptualized and research must be geared toward solving practical communication problems such as improving group relationships between black and white students and between students and college administrators. By "maximizing relevance" speech teachers and speech scholars can contribute to social change and the solution of man's communication problems. (DS)

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approaches to inquiry in communication

by Samuel L. Becker

One of the complaints that I hear most often in the hallways, bars, and meeting rooms at conventions, is the degree to which communication research is stifled by the traditional power structure in departments of speech. I also hear criticisms of historical and critical studies because they add little to our understanding, predictive power, or ability to manipulate communication processes in desired ways. I would suggest that the major problem of those of us in speech who think of ourselves as behavioral scientists is not the critics outside communication research, but rather the quality of the research inside. I would also suggest that before we criticize the utility of the research of others, we be certain that our research has the utility which we claim for it.

Graduate students at the University of Iowa, as at most institutions I am sure, are often exceedingly perspective. A few years ago, a number of them compiled a list of suggestions on how to get a research project accepted for a thesis. (One of my colleagues collected these on a Friday afternoon in one of the local bistros.) I find some of these suggestions rather revealing.

1. Attack a published work, especially one of Aristotle's.
2. Show the relationship between your study and studies in other disciplines; the more disciplines, the better.
3. Show the need for a new specialized vocabulary for the execution of the project.
4. Make the obvious obscure.
5. Have an hypothesis which is not consistent with fact.
6. Have an hypothesis which is not consistent with human nature.
7. Have an hypothesis which is not consistent with itself.
8. Be certain that any findings of your study will be useless.
    a. Perhaps even harmful.
    b. Not consistent with tradition.
        (1) Christianity
        (2) The American Way.
        (3) The Ancients.

I am often forced to wonder about the extent to which many of our studies of communication have any sounder justification than these for being done. I believe that it may be worthwhile to consider the kinds of questions we are trying to answer with our communication research and the ways in which we arrive at these questions.

I contend that, in the past, we have spent far too much time trying to discover ways to answer questions and not nearly enough time developing questions which are *worth* answering.

David Bell, in a discussion of liberal education, has talked about the relative importance of knowing questions and answers.

> What is a question? A question, said Felix Cohen, is really an ambiguous proposition; the answer is its determination . . . The talmudic parable reverses the order of events: A man runs down the street shouting, "I've got an answer! Who has a question?" In the more esoteric versions, the parable reads: If God is the answer, what is the question?
>
> Which is the most difficult to find: the right question, or the right answer? In this—also a question—lies the heart of the educational inquiry.[1]

In this also, lies the heart of my concern. Bell's answer, and mine, is that the question is the more difficult to find and, hence, the more important. It is fairly easy to learn what to do *after* one has a question or hypothesis; this is probably the reason most of us concentrate our attention on that aspect of the research process. There are fairly clear rules and procedures for moving from question to techniques for data gathering and techniques for data analysis. There are no clear rules and procedures for the development of questions — questions which are both important and researchable. A good research question is much more than that bare grammatical structure which has a question mark at its tail. This is merely one component of a research question. It is also important to have a sound rationale for one's question—a statement of the reasons it is worth asking. This rationale indicates what will happen to other parts of knowledge or practice as a result of answering the question. This helps to distinguish between the consequential and the trivial question. It also helps to distinguish between questions which are relevant and questions which are irrelevant to a science of speech or communication (as opposed to a science of something else).

This location and definition of questions is where creativity is most essential to research. Some research has been likened to modern art, partly for this rea-

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son, and partly because both are not occupied with facts so much as with relationships—not occupied with numbers so much as with arrangements. One needs imagination which, coupled with a substantial knowledge of the phenomena to be studied, helps one to perceive patterns where others see disorder. Good research grows out of a combination of insight and fact, a constant movement back and forth between hypothesis and evidence, a game of leapfrog between imagination and knowledge. As Bronowski, who wrote about the creative process has said, "although science [or research] and art are social phenomena, an innovation in either field occurs only when a single mind perceives in disorder a deep new unity."

Any researcher has the responsibility of justifying the particular problem upon which he is working. Although I suppose one could argue that any bit of knowledge is better than none at all, no one but a fool would argue that all bits of knowledge are equally important—equally worth knowing. Of course, this raises problems for man today. We hesitate to make value judgements about such things—and with good cause. For what is the basis that we have today for saying that this is more important than that? We live in a world without Gods or demons. We live in a disenchanted world which has no philosophical, metaphysical, or theological beliefs to which we can turn for guidance on such questions. I have no generally accepted belief system to which I can turn for support of my decision that one research problem is more important than another. No behavioral scientist has. I can only argue that I believe certain problems are more important because of certain values to which I hold. You may disagree, and that is fine. But it is incumbent upon each of us, for our research, to clarify and justify the values to which we are committing ourselves with the selection of a particular type of research problem.

Political scientist Sheldon Wolin has talked about this problem in a discussion of Max Weber.

> As Weber puts it, 'The culture that we inquire into is a finite segment of the meaningless infinity of the world process, a segment on which human beings and human beings alone confer significance.' So here is the social scientist, unsupported by holy writ, unsupported by philosophical writ, unsupported by any other belief at bottom except his own conviction that he ought to inquire and, therefore, proceeding to inquire. I do not think it's unfair to view Weber as, in some very odd sense, an existentialist—perhaps the first extentialist social scientist—be[illegible] for Weber, every social science inquiry [illegible]n a commitment.[2]

The question for us in communication research, as in any other field of scholarship, is to what we will commit ourselves.

One of the first things to which we need to commit ourselves is a set of concepts around which a science of communication may be built. One of the reasons our progress has been slowed is that we became hung up in the early days of communication research on such concepts as emotional appeals and logical appeals—concepts so gross and vaguely defined that no one knew where to go with them. It took us too many years to begin to get these scaled down and defined in some scientifically meaningful ways, and the job is far from complete. Bowers is certainly making some progress in this area with his unique work on language intensity. Miller and Hewgill and others are beginning to get another dimension of emotional appeals clarified with their work on fear appeals.

Dresser and McCroskey and others are doing good work on clarifying one dimension of logical appeals, the use of evidence. However, far more work of this sort is needed. Each of us needs to attend more to the particular concepts on which he is or should be working. We must constantly ask ourselves about the fruitfulness of the concepts with which we are working. Are they essential to the explanation of something that can be understood in no other way? That is to say, are they essential to explain some pattern of relationships between disparate sets of circumstances, including some communication situations, and a set of related behaviors in which we are interested (such as voting behavior, signing petitions, giving money to some cause, refusing to rent one's house to an Arab, etc.)? We have too often failed to force ourselves to answer these potentially embarrassing questions.

A related aspect of our failure to properly conceptualize our research problems in communication, is that we have tended to neglect the question of the relationship of our particular research project to the communication problems in the "real world" to which we need to be able ultimately to generalize. As an applied field (and, contrary to many, I see nothing demeaning about that term)—as an applied field, our research ought to be leading toward answers to the communication problems that agitate our society. Each of us should be deeply disturbed when, after these many years of communication research, we have nothing constructive to say about means for improving communication between black and white, between college students and college administrators.

In looking at the research which we have done to date, I fail to see how we have any more to contribute to these problems than those individuals do

who simply use a bit of common sense. My point may be clarified with an analogy. If the research which has been done in the past three years in such applied fields as engineering and medicine were wiped out, our lives would be materially changed. On the other hand, if the research which has been done in the past three years in the equally important field of communication were wiped out, I cannot conceive that it would make the slightest difference in our lives.[3] If we are agreed that this descriptive statement bears a close relationship to reality, it is a damning indictment of our field.

How many of us, in considering the communication problems which need to be studied, begin with the communication problems of the 1960's? I am afraid that, even when we try to relate our work to the real world, we relate it to the world that is past. I hear young scholars talking about the behaviors of whites toward Negroes. Who is it who is beginning his thinking about communication research with the problem of communication variables related to the behaviors of Negroes toward whites? Who is it who is beginning his research by thinking about the relationships among Negroes—the problems of developing leadership in the ghettos, the problems of information diffusion in the ghetto, the problems of achieving consensus among the traditional and the new radical leaders in the ghetto? Even much closer to home for most of us, who is it who is beginning his research by worrying about the processes of innovation and diffusion of new ways of teaching people to communicate—in other words, ways to get speech teachers to try other than the traditional methods of teaching speech? Here is a problem right on our doorstep. What communication scholar has something to contribute to our understanding of this problem of communication and social change?

Once one accepts the point of view that ours is an applied field and that our research should be designed in such a way that the ultimate application of findings will be clear, another problem is thrust upon us. There is ample evidence that, for almost any meaningful change in human behavior, communication is only one relatively small set in a very large complex of involved variables. Communication works upon and is worked upon by a large host of other variables in affecting human behaviors. Thus, as scholars concerned with understanding the role of communication processes in the maintenance or change of behaviors, we must examine these processes in the context of these other variables. Though attempting to isolate communication variables may make for neater experiments, I am far from convinced that it leads to a better understanding of the usual ways in which communication works. As a matter of fact, quite the opposite may be true. For example, those of us who consider ourselves communication scholars are constantly talking about and writing about the *process* or *processes* of communication. We tell our students and anyone else who will listen that they must stop thinking of communication as a simple sort of phenomenon which can be stopped and examined or which can be conceptualized as anything but an interacting mass of variables which are constantly changing. This is the way we talk and write. But where is this idea of *process* when we come to do our research? I see us continuing to do the same sorts of research that communication scholars did before the idea of process had so much currency. I cannot help but wonder why we don't put our *concepts* where our mouths are?

I suspect that one of the reasons that we have not included the concept of process in our research is that none of our existing research techniques or designs "fits" that concept, and we cannot break ourselves loose from the technique with which we are familiar. We do not attempt to think through all of the possible ways to study the questions that concern us. We become interested in a particular technique—Q-sort, the sematic differential, or whatever—and insist upon designing our studies to fit these techniques, rather than the reverse. We forget that research does not consist exclusively of certain kinds of activities, whether laboratory experimentation or anything else. As one behavioral scientist has said,

> There are cookbooks on cookbooks of pat formulas and pat patterns of experimental design which, in many instances, become the researcher's Bible without its Protestant tradition of individual interpretation. For the intellectually lazy, it offers a safe refuge from the necessity for thought in conceptualizing the problem, considering alternative approaches to its solution, and testing tentative solutions against stark realities. The use of cookbook approaches in research can easily involve the substitution of an approved ritual for a required rigor.[4]

We in departments of speech are poised on the fore-edge of a critical period in the development of the field of communication research. For the first time since the birth of this area, and for the first time since the many long years of sickness and neglect when this infant field was kept alive only by the ministrations of attending doctors such as Franklin Knower and Howard Gilkinson, we have a sizable and lusty body of scholars doing enough research to make an important impact. It would seem to me wise at this time to look about to see whether

we are growing in the way that we should be growing, whether we are moving in the direction that we want to move.

I am certain we are in complete agreement that one of the general goals of our area is to understand communication processess as fully as possible. Though I am somewhat less certain, I suspect most of us would also agree that we want that understanding for a purpose, so that man can cope with his environment more effectively. If we are in agreement on those points, we have some basis for assessing where we have been and for setting directions in which to go from here. None of what I have said, however, should be taken to mean that there are neat, clearly-understood ways of making this assessment or setting directions or planning research or doing that research. There is much misunderstanding of the scientific method on c is score. It is not the consistent, well-organized set of procedures that so many persons assume. As physicist Percy Bridgman has said, in discussing the scientific method, "the scientist has no other method than doing his damnedest." I agree. What I question today is whether we are doing our damnedest to build a science which is of maximum relevance for an understanding of the major problems of communication that confront man.

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Samuel L. Becker is Chairman of the Department of Speech and Dramatic Art at the University of Iowa. He received his B.A., M.A., and Ph.D. from the University of Iowa and did post-doctoral work in Sociology at Columbia University. In 1958 he was awarded a Mass Media Fellowship from the Fund for Adult Education for study at Columbia University, did work at the Bureau of Applied Social Research, and a study of computer programming at the Watson Laboratory. In 1963, he received a Fullbright Professorship to lecture at the University of Nottingham (England) on educational television and, primarily to do a study for them on the diffusion of the use of television in the schools of England. Prior to his present appointment, he taught at the Universities of Wyoming, Wisconsin, and Nottingham.

Professor Becker is presently Editor of *Speech Monographs* and a member of the Executive Committee of the Speech Association of America. At one time or another, he has held the following offices: Chairman of the Publications Committee of SAA; Chairman of the Behavioral Science Interest Group and the Radio-Television-Film interest group of SAA; a member of the Board of Directors of the National Association of the Educational Broadcasters; Chairman of the Research Committee of NAEB; President of the Iowa Chapter of the AAUP; Editorial Boards of *Speech Monographs* and *Central States Speech Journal:* and has served on the Board of Directors of Goodwill Industries of Southwest Iowa.

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